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Taxation, non-tax revenue and democracy: New evidence using new cross-country data

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ABSTRACT

A large body of econometric research has generated growing support for the existence of a political resource curse, but has nonetheless continued to be regularly punctuated by research contesting those conclusions. This continuing disagreement can be explained in significant part by problems associated with low-quality government revenue data: it has undermined the robustness of many existing findings, while leading other researchers to rely on alternative measures of resource income as their primary explanatory variable – a highly imperfect measures of the underlying relationship of interest. We re-examine the relationship between taxation, non-tax revenue and democracy by employing dramatically improved data developed specifically for this research. We find the strongest evidence to date of a political resource curse, and provide evidence about the specific details of the underlying relationship: (i) natural resource wealth is anti-democratic, rather than merely stabilizing; (ii) it is driven primarily by changes in the composition of government revenue; (iii) it is best understood as a long-term relationship, rather than short-term changes in resource wealth being quickly translated into major political changes; and (iv) it is driven primarily by oil wealth, rather than mineral wealth, because governments are comparatively effective at translating oil wealth into the government revenues that drive the political resource curse.

1. Introduction

Most research into the political resource curse proposes that governments that rely more heavily on revenue from non-renewable natural resources[1], and which are less reliant on national taxation, are less likely to be democratic and accountable to their citizens. Building on this research, resource dependence has become a prominent feature of accounts in political science and economics about the long-term drivers of state building, institutional change and democracy. This argument has been based largely on cross-country econometric studies, which have proliferated over the past 15 years, with most cross-country econometric research reporting the expected negative relationship between natural resource wealth and democracy.

However, among the studies investigating "whether the resource curse is real or illusory", a significant group have continued to contest these results (Ross, 2015: 240). Some have argued that findings of a political resource curse are simply not robust (Haber & Menaldo, 2011). Meanwhile, recent work by Morrison (2009, 2015) – building on earlier work by Smith (2004) – has proposed an alternative interpretation: that natural resource wealth, and non-tax revenue more generally, is not anti-democratic, but tends to stabilize democracies and non-democracies alike, while taxation has the opposite effect. This divergence within existing results is puzzling: Why have multiple researchers, asking a consistent and well-defined research question, over almost two decades, failed to arrive at more consistent findings?

In this paper we argue that a significant part of existing disagreement can be explained by a combination of low quality data and correspondingly mis-specified tests of the relationship of interest. We demonstrate that after correcting for these two problems there is far more consistent and persuasive evidence of the existence of the political resource curse.[2] Furthermore, we are able

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[1] Primarily oil and, to a lesser extent, other mineral resources.

[2] Some of the findings of this paper have been previously described in Prichard (2016a), which referred to an earlier Working Paper version of this paper.

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0305-750X/

to add clarity about underlying mechanisms that have remained debated in the literature. First, that the effects of natural resource revenues are anti-democratic, rather than merely stabilizing. Second, that the political resource curse is driven by *changes in the composition of government revenue*, rather than alternative mechanisms linking resource extraction more broadly to reduced democracy. This is important in its own right, and helps to explain recent evidence that the political resource curse is driven primarily by oil wealth rather than mineral wealth (Ross, 2012): oil wealth tends to be much more effectively translated into expanded government revenue. Third, that the political resource curse is best understood as a long-term relationship, rather than one in which levels of democracy are highly sensitive to short term fluctuations in resource revenues (Ross, 2015).

The starting point for our analysis is a return to the theory underpinning the political resource curse. The most compelling and commonly cited version of the political resource curse hypothesis focuses on the impact of changes in the *composition of government revenue* on political outcomes.[3] Stated most broadly, government reliance on non-tax revenue – that is, government revenue from comparatively captive sources, and primarily (but not exclusively) from non-renewable natural resources[4] – is expected to reduce democracy and accountability by weakening state-society links, facilitating government investments in patronage and repression, and driving expanded political corruption (Ross, 2001). Meanwhile, reliance on tax revenue – that is, revenue raised from relatively broad-based taxes on individual taxpayers and businesses – may have a conversely positive impact on governance by providing the state with stronger incentives to "bargain" with their citizens over how public revenue is used and the broader extent of political representation (Moore, 1998; Prichard, 2015).

However, owing to data limitations there has yet to be a study that convincingly and directly tests the relationship between the composition of government revenue and democracy. A handful of studies have set out to test this relationship directly, but have been undermined by severely inadequate government revenue data (Ross, 2004; Mahon, 2005; Morrison, 2009, 2015). This data, primarily from the IMF and World Bank, has suffered from extensive missing observations and has failed to distinguish effectively between tax and nontax revenues. Owing to these data limitations, most studies of the political resource curse have instead tested the relationship between *resource income*[5] and democracy. However, while this is an intuitive proxy for changes in the composition of government revenue, it is highly imperfect, and thus fails to precisely test the key mechanism underpinning the most persuasive version of the resource curse hypothesis (Wiens, Poast & Clark, 2014).

In what follows we address these longstanding problems by drawing on dramatically improved data from the ICTD Government Revenue Dataset (GRD), which was constructed explicitly, though not exclusively, for this project (Prichard, Cobham, & Goodall, 2014).[6] These improved data are pivotal. Table 1 lists the 33 resource dependent states that collect at least 10% of GDP in non-tax revenue[7] and for which any data is available. It then compares data coverage here to data coverage in the two most high profile papers to have previously run similar tests linking the composition of government revenue to levels of democracy (Ross, 2004; Morrison, 2009).[8] The table notes countries for which there was either (a) no data in earlier studies, (b) extremely limited data, or (c) data that was analytically problematic owing to a failure in earlier international datasets to adequately distinguish between normal tax revenues and resource revenues.[9]

The limitations of those early papers are immediately apparent. A seminal paper by Ross (2004) exploring the connections between tax reliance and democracy contains no data for nine of the 33 countries, limited data for six countries, and contains data that is analytically problematic – and often severely so – for an additional fourteen countries. Data is relatively complete, and entirely free of problems, for only four of 33 countries. More recent work by Morrison (2009) rightly excludes the most analytically problematic data, but at the expense of extremely limited data coverage: His dataset contains no data for fifteen of these countries, very limited data for an additional five, and analytically problematic data for seven more countries. Relatively complete and accurate data is available for only seven of 33 resource-dependent countries. While these data problems merely reflect the limits of earlier datasets, and efforts by the authors to ask important questions while employing the best available data at the time, they raise serious concerns about the reliability of earlier results.

With access to this new data we implement three distinct sets of econometric tests: Generalized Method of Moments (GMM), Mean-Group Estimators (MG), and Random and Fixed-Effects Logit estimators. All of the estimators yield clear support for the existence of a political resource curse that is both statistically significant and large in magnitude. In turn, we are able to explicitly demonstrate that, consistent with theory, all of the results are stronger when focusing on the *composition of government revenue*, rather than *resource income*, while the results are similarly stronger when employing estimators that focus on the long-term relationship between revenue and democracy, rather than focusing exclusively on short-term changes.

The paper proceeds in seven parts. The next section presents a brief review of the relevant literature. The second section presents the new data and the construction of the revenue variables. The third section presents the empirical strategy, reviewing the rationale for a range of alternative econometric models. The fourth section presents the core results and the fifth section presents robustness checks. The sixth section offers a discussion of the core results. The final section concludes.

2. Going back to basics: Model specification and data

Most studies of the political resource curse propose that natural resource wealth – and other forms of non-tax revenue – are likely to undermine the quality of a country's governance by disconnecting governments from their citizens, supporting the expansion of corruption, patronage and repression, and increasing the risk of conflict (Ross, 2015). Beginning with Ross (2001), a wide array of cross-country econometric studies have reported support for such

[3] Natural resource wealth – and oil wealth in particular - may also affect democracy through alternative channels, detailed most completely in Ross (2012). However, our contention is that revenue related channels are the primary mechanisms linking resource wealth to autocracy.

[4] As described in detail later, this definition is rooted in the political economy literature, and differs from a purely accounting definition.

[5] Defined as the total annual value of resource production, either per capita or as a share of GDP.

[6] We employ the May 2016 version of the data, available at http://www.ictd.ac/datasets/the-ictd-government-revenue-dataset. Future updates of the data will be hosted by UNU WIDER, and are available here: https://www.wider.unu.edu/project/government-revenue-dataset. An earlier working paper version of this paper reported results emplying the original 2014 version of the ICTD data, while the 2016 version of the data, used here, is significantly improved in terms of coverage, accuracy and the length of the time series.

[7] Of which the majority is from non-renewable natural resources.

[8] Haber and Menaldo (2011) construct a similar fiscal reliance variable, but it covers only 14 of the 33 countries listed here. Morrison (2015) provides a more recent version of his 2009 results. It was unfortunately not possible to access that data, but the sample sizes are almost identical to those from the 2009 paper.

[9] This issue is discussed at length later in the paper. Data is labeled "analytically problematic" if the share of tax revenue in total government revenue reported in the original source is at least 50% larger than the level reported in the more accurate ICTD GRD.

Table 1
Comparative data coverage for resource dependent states.

	ICTD GRD	Ross (2004)	Morrison (2009)
Angola	Included	Omitted	Omitted
United Arab Emirates	Included	Omitted	Omitted
Azerbaijan	Included	Omitted	Omitted
Bahrain	Included	Analytically problematic	Analytically problematic
Bolivia	Included	Analytically problematic	Included
Brunei	Included	Omitted	Omitted
Bhutan	Included	Included	Included
Botswana	Included	Analytically problematic	Omitted
Republic of the Congo	Included	Analytically problematic	Limited data
Algeria	Included	Limited data, analytically problematic	Omitted
Ecuador	Included	Analytically problematic	Omitted
Egypt	Included	Analytically problematic	Analytically problematic
Gabon	Included	Analytically problematic	Analytically problematic, no data after 1991
Equatorial Guinea	Included	Omitted	Omitted
Iran	Included	Analytically problematic	Analytically problematic
Iraq	Included	Omitted	Omitted
Kazakhstan	Included	Omitted	Included
Kuwait	Included	Included	Included
Libya	Included	Omitted	Omitted
Mongolia	Included	Included	Included
Mauritania	Included	No data after 1980	Omitted
Malaysia	Included	Analytically problematic	Analytically problematic
Nigeria	Included	No data after 1987	Omitted
Norway	Included	Included	Included
Oman	Included	Analytically problematic	Omitted
Russian Federation	Included	Limited data, Analytically problematic	Limited data
Saudi Arabia	Included	Omitted	Omitted
Sudan	Included	No data after 1982	Omitted
Syria	Included	Analytically problematic	Analytically problematic
Chad	Included	No data after 1991	No data after 1991
Trinidad and Tobago	Included	Analytically problematic	Limited data, analytically problematic
Venezuela	Included	Analytically problematic	Analytically problematic
Yemen	Included	Analytically problematic	Analytically problematic

Notes: Excludes recently independent Timor-Leste and South Sudan.

a negative relationship, focusing particularly on the impact of natural resource wealth on levels of democracy (Wantchekon, 2002; Jensen & Wantchekon, 2004; Ross, 2004; Tsui, 2010; Aslaksen, 2010; Ramsay, 2011; Ross, 2012; Andersen & Ross, 2014; Wiens et al., 2014; Ahmadov, 2014; Lall, 2017).

However, a smaller group of researchers have continued to question the robustness of these results. The most high profile challenge has come from Haber and Menaldo (2011), who constructed a new cross-country data set of national resource income dating back to 1800, and reported no consistent support for the existence of a political resource curse. Elsewhere, critics have argued that the apparent relationship may be driven by endogeneity or mismeasurement, or may be offset by countervailing positive effects of resource wealth (e.g. Brooks & Kurtz, 2016; Liou & Musgrave, 2014; Bruckner, Ciccone & Tisei, 2012; Wacziarg, 2012; Dunning, 2008; Herb, 2005)[10]. Meanwhile, Morrison (2009, 2015) and Smith (2004) have argued that natural resource wealth is not inherently anti-democratic but, instead, tends to increase regime stability – while Morrison has argued that higher taxation has the opposite, destabilizing effect.

While these conflicting findings are indicative of a rich intellectual debate, their inconsistency is also a source of concern: Why, despite a well-defined research question, do we not see greater empirical agreement across studies? Recent debates have tended to focus particularly on disagreements about appropriate econometric methods, and appropriate country samples, but have failed to yield agreement (see, for example, Haber & Menaldo, 2011; Andersen & Ross, 2014; Wiens et al., 2014). By contrast, we focus on a much simpler possibility: that conflicting results reflect basic, but foundational, problems of model specification and data. The focus on data problems mirrors the intuition in Lall (2017), who employs multiple imputation to fill gaps in major datasets. However we go further, by introducing new data rather than relying on imputation, and adopting more appropriate empirical measures of the concepts of interest.

The most prominent theories of the political resource curse do not focus on the political implications of resource *wealth* in general, but focus more specifically on the political implications of differences in the *composition of government revenue* – driven, in particular, by revenue from non-renewable natural resources. This analytical focus on government revenue is rooted in part in early accounts of autocracy in resource rich states (Mahdavy, 1970; Beblawi & Luciani 1987). Its deeper roots lie in the broad tradition of fiscal sociology, which proposed that the character of states could be explained in significant part by differences in the sources of government revenue – and, in particular, by differences between states that could rely on captive sources of non-tax revenue, and states that needed to rely on the taxation of their own citizens.[11]

This relationship is most commonly attributed to two related, but distinct, mechanisms. On one hand, governments with access to captive sources of non-tax revenue, primarily from natural resources, will be empowered to resist political opposition through repression or through expanded public spending and patronage. On the other hand, governments that rely heavily on tax revenue will need to bargain with mobile taxpayers, conceding greater accountability and democracy in exchange for tax payments (Tilly, 1992; Levi, 1988). Ulfelder (2007) and others refer to these

[10] Brunnschweiler and Bulte (2008) and Alexeev and Conrad (2009) make a similar argument, though with a focus on explaining alternative dependent variables.

[11] The most famous scholar in this tradition was Joseph Schumpeter. For a summary see Moore 2004.

alternatives as the "supply-side" and "demand-side" of the political resource curse, respectively.

However, most contemporary econometric tests of the political resource curse have *not* focused on the impact of *resource revenue* on democracy, but instead on the connections between the *total value of resource production ("resource income")* – either as a share of GDP or in absolute terms – and democracy. This is a critical theoretical distinction. A government that is able to capture extensive *revenue* from the exploitation of natural resource wealth will be able to spend that revenue on patronage and repression, while it will have less need to bargain with taxpayers. Critically, the same effects will *not* hold true to the same extent where a country has high *resource income*, but where it captures only a smaller proportion of resource rents as *government revenue*. This is, in turn, empirically significant: resource income is an imperfect substitute for measuring the composition of government revenue, both because the translation of resource wealth into resource revenue varies across resources and countries[12] and because focusing on resource income entirely ignores the level of non-resource tax collection – that is, the "demand side" of dominant theories of the resource curse.

Tellingly, there is broad agreement on the theoretical appropriateness of employing measures of the composition of government revenue – even among some researchers who have relied primarily on measures of resource income. Haber and Menaldo (2011) construct a measure of "fiscal reliance", defined as the share of natural resource revenue in total government revenue, but it covers only 19 countries owing to difficulties of gathering relevant data. Wiens, et al. (2014: 786) note explicitly that government revenue is "our theoretical quantity of interest", but that they rely on measures of resource income for reasons of practicality. Why do we observe this disconnect between theory and practice? Most simply, the low quality of previously available data on government revenues has led most existing researchers to rely on measures of resource income, which have been more readily available and complete. Meanwhile, as discussed in the introduction, the robustnesss of the results from a handful of studies that have employed government revenue data are open to question owing to data problems (Ross, 2004; Morrison, 2009, 2015).

Against this background, the central contribution of this study lies in introducing the ICTD GRD, which contains more complete and accurate data on the composition of government revenue. This allows us to directly test the impact of changes in the composition of government revenue on levels of democracy, thus more precisely capturing the core theoretical propositions underpinning the political resource, and increasing the accuracy of our resultant estimates.[13]

3. Data and variables

The ICTD GRD has several key advantages relative to earlier government revenue datasets, of which two are most critical: Improved coverage, and a more consistent distinction between tax and non-tax sources of revenue.[14] Previous research has relied primarily on data from the IMF and World Bank, both of which have historically been plagued by missing data (Ross, 2004; Morrison, 2009, 2015), or on datasets that offer relatively more complete data coverage for only a very limited subset of countries or years (Haber & Menaldo, 2011; Baskaran & Bigsten, 2013). By contrast, the ICTD GRD covers 188 countries and a total of 3763 country-year observations during the period 1990–2012.[15] This was, at the time of the analysis, almost 70% more developing country observations than the IMF Government Finance Statistics during the same period.[16]

At least as important are substantial improvements in the analytical accuracy of the data, particularly through the creation of a consistent distinction between tax and non-tax revenue.[17] These improvements are, in turn, pivotal to improvements in data coverage for resource rich states. Such states are critical to any test of theories linking government revenue and democracy. However, because earlier data did not consistently allow for distinguishing between resource and non-resource sources of revenue, resource rich states have been systematically under-represented in the data underpinning existing studies, as illustrated earlier in Table 1.

At its simplest, the resource curse hypothesis distinguishes the political effects of two types of revenue: revenue from natural resources, and all other types of government revenue. Moore (1998) provides a slightly more general formulation, distinguishing between "unearned" revenue, which is collected from narrow and relatively captive sources (primarily, but not exclusively, non-renewable natural resource revenues), and "earned" revenue, which is collected from a broader base of individual taxpayers and businesses.[18] This has, in turn, been operationalized in existing studies as a distinction between "tax revenue" ("earned") and "non-tax revenue" ("unearned") (Morrison, 2009, 2015; Ross, 2004).

Unfortunately, this operationalization in earlier studies has been empirically problematic. In general, governments collect resource revenues in two ways: (i) corporate taxes on resource firms and (ii) various types of royalties and levies.[19] From a political economy perspective, both types of revenue should be classified as "non-tax revenue", as they are captive types of revenue expected to reinforce government power. However, following an accounting logic, international databases have generally recorded the former as "taxes", and the latter as "non-tax revenue"[20] – thus obfuscating the distinction between "earned" and "unearned" revenue relevant for testing the existence of the resource curse.

[12] The most important variation is between oil and mineral resources, with the former generally translated into significantly greater government revenue. Variation may equally come from the ease with which resources can be extracted, the nature of the contracts governing resource extraction, or simple administrative effectiveness in collecting revenues due.

[13] Some have argued that this approach may suffer from problems of endogeneity: low-income countries may be highly dependent on resource revenue not because the resource sector is large, but because the rest of the economy is small and tax collection is weak (Dunning, 2008; Brunnschweiler & Bulte, 2008). These authors have favored employing absolute measures of resource wealth per capita. However, this seems unwarranted here: Doing so distorts the research question (which is fundamentally about the *relative* share of resource wealth), prevents measuring the revenue driven causal process precisely, and is only relevant if income is a significant predictor of democracy, which recent studies call into question – see, for example, Acemoglu et al. (2008).

[14] A more detailed discussion is available in Prichard, Cobham and Goodall (2014).

[15] We employ the preferred "merged" ICTD GRD dataset, which employs general government data for fiscally decentralized states, and more widely available central government data otherwise. A small number of potential observations are excluded from the analysis owing to (a) an inability to deal effectively with resource revenues, (b) highly irregular and questionable data, or (c) analytical incomparability. These exclusions are recommended in the guide to the dataset, and are described in detail in Prichard, Cobham and Goodall (2014).

[16] The analysis here focuses on data beginning in 1990, as it offers the most complete coverage. As demonstrated in the Appendix, the results are nonetheless robust to employing earlier data. It is important to note that the IMF GFS is constantly evolving – and, recently at least, improving – and these figures are thus accurate as of the time of the creation of the version of the ICTD GRD on which we rely.

[17] Additional improvements result from employing a consistent GDP series, removing clearly implausible or analytically problematic data, and standardizing the treatment of social contributions. See Prichard, Cobham and Goodall 2014.

[18] The primary component of "unearned" revenue is resource revenues, but this category also includes other types of captive revenue - state owned enterprises, property income, investment funds and the like, thus providing a more complete picture of the composition of government finances.

[19] For simplicity these terms are used broadly to include royalties as such, profit sharing, the auction values of exploration and extraction rights and any other legal mechanism – other than taxes – for transferring natural resource rents to the government.

[20] This describes the general pattern, but each country is somewhat unique, and the ICTD GRD has correspondingly 'corrected' the data country-by-country.

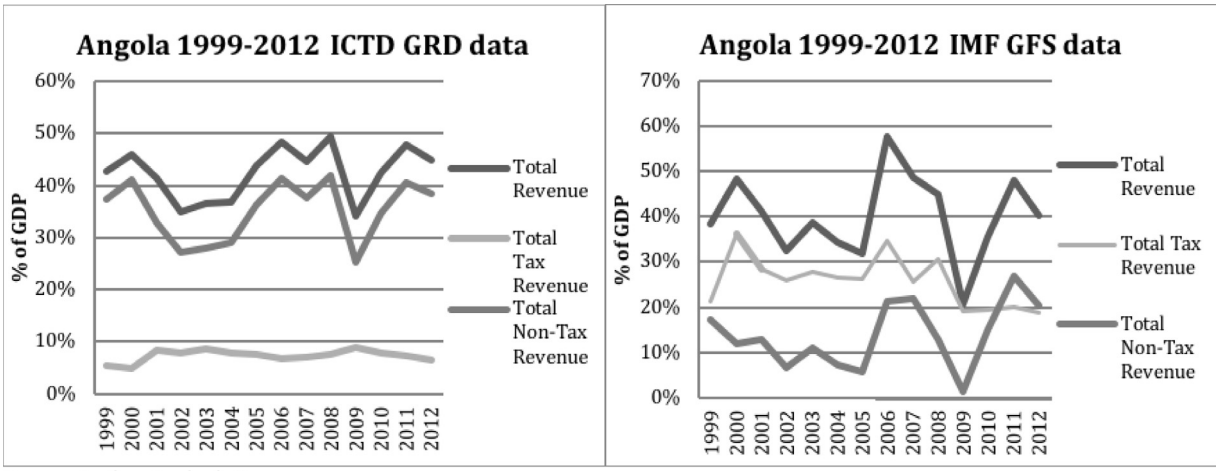
	Tax Revenue	Non-Tax Revenue
IMF GFS	Non-Resource Direct Tax Non-Resource Indirect Tax **Corporate Taxes on Private Resource Companies**	Resource Non-Tax Revenue Non-Resource Non-Tax Revenue
ICTD GRD	Non-Resource Direct Tax Non-Resource Indirect Tax	Resource Non-Tax Revenue **Corporate Taxes on Private Resource Companies** Non-Resource Non-Tax Revenue

Fig. 1. Re-categorization of resource revenues in ICTD GRD.

Table 2
Corrected data in the ICTD GRD: Illustrations in levels.

	Source	Total revenue	Total tax	Total non-tax revenue	Tax reliance as share of government revenue
Angola 2002	ICTD GRD	35.1%	7.8%	27.3%	0.222
	IMF GFS	35.6%	28.4%	7.2%	0.798
Kazakhstan 2010	ICTD GRD	23.9%	12.7%	8.0%	0.531
	IMF GFS	22%	19.3%	2.7%	0.877

Source: Authors' calculations. IMF GFS data downloaded in July 2015.



Source: Authors' calculations.

Fig. 2. Corrected data in the ICTD GRD: Time Series Illustration.

The key innovation of the ICTD GRD has correspondingly been to allow us to re-classify corporate taxation of resource firms as "non-tax revenue", in order to align our empirical measures with theory.[21] This shift is illustrated in Fig. 1, while Table 2 and Fig. 2 provide examples of the impact of this shift on the data reported for individual countries by comparing the ICTD data to data from the IMF GFS[22], illustrating both effects in levels and the benefits of the ICTD GRD in reducing misleading volatility in the data over time.[23] These benefits of re-classification are in addition to the large increase in data coverage, noted above, that are allowed for by this classification method. Of course, the resultant data still suffers from standard concerns about measurement error or misreporting when using self-reported government data. It is, in particular, possible that there may, in some cases, be underestimation of non-tax revenues where they flow off-budget, as may be the case in some large and relatively un-transparent resource producing states. However, systematic comparison across time and data sources gives us significant confidence that these concerns are, at worst, relatively modest and that the data tells a much more accurate story than earlier options.

Having made this adjustment to the data, we are able to construct measures of our concepts of interest that are more analytically accurate than previous studies. The first variable captures total tax collection as a share of GDP (*tottax*) *exclusive* of resource revenues, thus making it a more consistent proxy for "earned" income. The second variable captures total non-tax revenue, inclusive of *all* resource revenues but excluding grants,[24] as a share of

[21] This process draws primarily on IMF Article IV Reports. Occasional country-year observations for which this distinction cannot be successfully drawn are excluded from the analysis.

[22] Data as of 2015, when the version of ICTD GRD that we employ was downloaded.

[23] The Angolan case is relatively extreme in its magnitude, but for this reason serves as a particularly clear illustration. The case of Kazakhstan is of a magnitude more comparable to other cases.

[24] We exclude aid grants from non-tax revenue owing to problems of data availability and accuracy, and reflecting significant evidence elsewhere that aid may affect government incentives differently than natural resource rents. See, for example, Morrissey, Prichard and Torrance (2014).

GDP (*totnontax*), and is a proxy for "unearned" income.[25] Finally, we are able to combine these two variables to construct a measure of tax reliance (*tax_rel*), which measures the share of non-resource tax revenue in total government revenue.[26] All of our econometric tests first look at the impact of tax reliance (*tax_rel*) on democracy, following Ross (2004), after which we disaggregate tax reliance into its component parts, *tottax* and *totnontax*, in an effort to disentangle their respective roles, following Morrison (2009, 2015).

Our primary dependent variable is *Democracy*, which is constructed by employing the Polity2 measure of democracy, and normalizing it to the range 0–100 (Marshall & Jaggers, 2008).[27] In our logit regressions, we employ a binary measure of democracy, *Regime*, with democracies coded 1, drawn from Cheibub, Ghandi and Vreeland (2010). Owing to significant questions about the robustness of different predictors of democracy, we include a relatively limited set of widely used control variables in our core results. The most widely cited predictor of accountability remains log GDP per capita (*lgdp*), which we draw from the World Economic Outlook database.[28] We equally include a measure of economic growth, from the World Development Indicators, to capture more proximate economic performance (*Growthpc*). Finally, we include a dummy variable for the occurrence of violent conflict (*Civil_War*), which we draw from Haber and Menaldo (2011) and extend to 2012 using the UCDP/PRIO Armed Conflict Dataset. In the Appendix we show robustness to different constellations of these variables, as well as to two less widely used variables employed by Haber and Menaldo (2011): population (*Pop*), and regional democratic diffusion (*Regional_Dem_Diffuse*).

Figs. 3, 4 and 5 present simple correlations between average values over the period 1990–2012 of our core dependent variable, *Democracy*, and our three alternative revenue measures: *Tax_rel* (tax reliance), *Tottax* (total tax revenue) and *Totnontax* (total non-tax revenue).

As expected, countries that are more reliant on tax revenue, and less reliant on non-tax revenue, tend to be more democratic. Particularly striking are patterns among resource producers. There are 18 countries[29] that have collected an average of at least 13% of GDP in non-tax revenue, and only two – Botswana and Timor Leste – have been even weak democracies, as judged by having an average score of at least 50 (out of 100) on our adjusted polity variable[30]. Still more striking, of 29 countries[31] for which less than 60% of total government revenue comes from non-resource taxes only 3 have, on average, met even the very limited definition of democracy above (Venezuela, Botswana and Timor Leste).

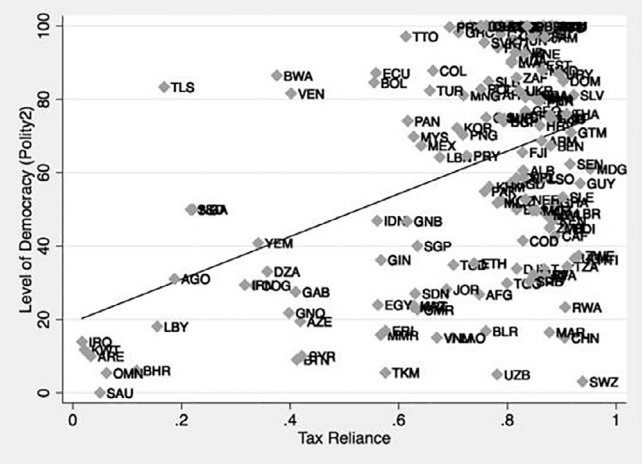


Fig. 3. Relationship between tax reliance and polity using country averages, 1990–2012.

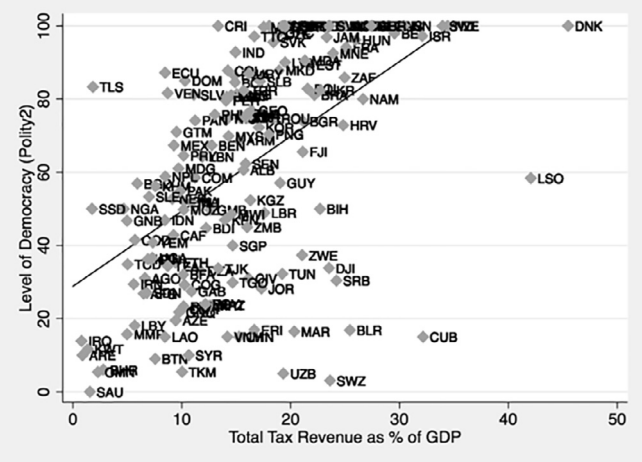


Fig. 4. Relationship between total tax revenue and polity using country averages, 1990–2012.

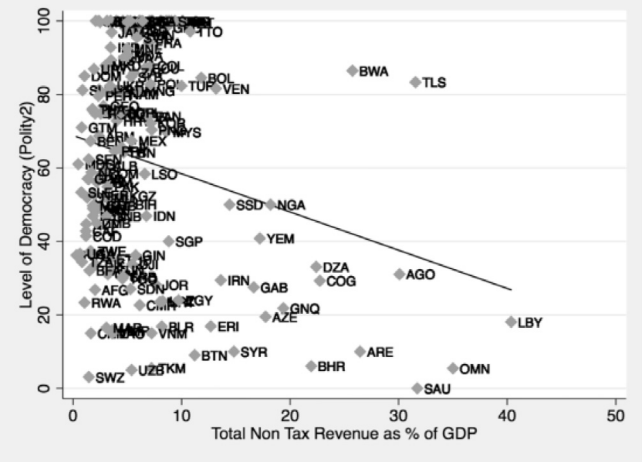


Fig. 5. Relationship between total non-tax revenue and polity using country averages, 1990–2012.

[25] An argument could be made for focusing exclusively on natural resource revenue, as opposed to broader non-tax revenue. There are two reasons why we do not. First, pragmatic: consistent data on resource revenues specifically is not sufficiently widely available, while the vast majority of variation in non-tax revenue is, in any event, explained by resource revenues. Second, conceptual: Among the other primary drivers of variation in non-tax revenue are other "unearned" revenue sources, including revenue from state investment funds, state monopolies and enterprises, and fishing or forestry licenses.

[26] Mathematically, *tax_rel* = *tottax* / (*tottax*+*totnontax*)

[27] The raw polity2 measure runs from -10 to 10. Normalization involves treating each 1 point increment in the raw data as a half point increment from 0 to 10, and then multiplying by 10.

[28] For consistency, as this is the same figure employed in the ICTD dataset.

[29] Angola, UAE, Azerbaijan, Bahrain, Botswana, Republic of the Congo, Algeria, Gabon, Equatorial Guinea, Iran, Iraq, Kuwait, Libya, Nigeria, Qatar, Saudi Arabia, Timor-Leste, Yemen.

[30] Equivalent to a 0 for the unadjusted polity2 variable from the Polity IV dataset.

[31] Angola, UAE, Azerbaijan, Bahrain, Bhutan, Botswana, Republic of the Congo, Algeria, Ecuador, Egypt, Eritrea, Gabon, Guinea, Guinea-Bissau, Equatorial Guinea, Indonesia, Iran, Iraq, Kuwait, Libya, Myanmar, Nigeria, Qatar, Saudi Arabia, Syria, Turkmenistan, Timor-Leste, Venezuela, Yemen.

4. Empirical strategy

Seeking to move beyond simple correlations, we estimate several alternative econometric models that draw on the existing literature: A System-GMM panel estimator, a Mean Groups estimator to more fully exploit the time series dimension of our macro panel and, finally, random and fixed effects logit models designed to focus attention on the likelihood of transitions to (or from) democracy. Each estimator is subject to particular, but distinct, limitations. We view their parallel implementation as the best means to account for potential weaknesses in any individual estimation strategy, while also shedding light on underlying features of the relationship between revenues and democracy.[32]

4.1. Pooled OLS, fixed effects and general method of moments

We begin with the following simple dynamic specification:

$$Democracy_{i,t} = \delta Democracy_{i,t-l} + \beta_1 Tax_r el_{i,t-l} + X'_{i,t-l}\gamma + \alpha_i + \lambda_t + e_{i,t} \quad (1)$$

In the basic specification $Democracy_{i,t}$ is for country i in period t. On the right-hand side of Eq. (1) we include a lagged value of the dependent variable both to model the persistence of democracy over time and to account for potential mean reverting dynamics. The key independent variable of interest is the lagged value of Tax_rel_i. In our main specifications we consider a single year lag, t-l where l = 1, while we later run robustness checks using $l = 3$ and $l = 5$. The estimated parameter β_1 captures the relationship between tax reliance and the extent of democracy. The vector $X'_{i,t}$ includes our time-varying control variables. We add country fixed effects, α_i, to control for omitted country-specific factors, and time dummies, λ_t, to control for time-varying shocks that are common across countries. In all regressions, the error term, $e_{i,t}$, captures all other omitted factors.

After initially estimating the model using *tax_rel* as our independent variable, we disaggregate tax reliance into its component parts – tax revenue as a share of GDP (*tottax*) and non-tax revenue as a share of GDP (*totnotax)* – in order to attempt to disentangle competing arguments about the links between revenue and expenditures. Thus the second specification is:

$$Democracy_{i,t} = \delta Democracy_{i,t-l} + \beta_1 tottax_{i,t-l} + \beta_2 totnontax_{i,t-l} + X'_{i,t-l}\gamma + \alpha_i + \lambda_t + e_{i,t} \quad (2)$$

An important concern in relation to the tax reliance (*tax_rel*) and total tax revenue (*tottax*) variables is the possibility of reverse causation: while increased taxation may drive increased democracy, increased democracy might also lead to increased taxation (Levi, 1988; Timmons, 2010). This concern does not appear to apply to non-tax revenue, as there is no clear reason to expect democracy to shape non-tax revenue collection. Ross (2004: 238) argues that the use of a lagged dependent variable is the best available means to address the problem of reverse causality, based on the assumption that, "a change in the independent variables (taxes and government services) should precede a change in the dependent variable (regime type)." We thus initially follow this advice from Ross, before also implementing dynamic panel data methods explicitly designed to deal with potentially endogenous regressors. It is important to note that, as described in Bellemare, Masaki and Pepinsky (2017: 958), the GMM methods we use are not immune to endogeneity bias. However, Bellemare, et al. (2017) do find that they perform better than alternatives, thus making them a nonetheless instructive set of tests when paired with the other models that we estimate.

For the sake of completeness, we begin by following Ross (2004) in implementing a simple Pooled OLS model with lagged dependent variables. However, the Pooled OLS model omits country fixed-effects, and may suffer from omitted variables bias. We subsequently run a fixed effects (FE) model in order to control for time-invariant country heterogeneity. However, FE estimates that include a lagged dependent variable as a regressor, as in our case, are biased because of the correlation between the lagged dependent variable and the error term (Nickell, 1981). Meanwhile, by focusing exclusively on short-term within country variation and excluding cross-country variation, the FE model is particularly restrictive in modeling the relationship of interest.

In order to estimate a dynamic process with fixed effects we thus prefer to follow Aslaksen (2010) and others in employing GMM estimators that use instruments generated from the time series. The GMM models are considered the best estimator for dynamic processes with fixed effects where there are potentially endogenous regressors, and idiosyncratic disturbances. Difference-GMM uses first differences as instruments, while system-GMM uses both first differences and lagged levels. Sys-GMM is preferable when – as in this case – there is a high degree of persistence of the variables of interest (Blundell & Bond, 1998). By using lagged levels of the variables as instruments Sys-GMM preserves information on cross-country differences that is lost when only the first differenced equation is estimated, and is therefore more efficient (Aslaksen, 2010).

4.2. Mean group-common correlated effects (CCE-MG) estimator

While Sys-GMM is widely used, it is not without limitations. Alongside not strictly removing concerns about endogeneity, its comparative advantage is with large-N but small-T datasets, whereas we have up to 23 years of data from 1990 to 2012. In our view the most appropriate method for exploiting the time series dimension of the data is to use the mean group-common correlated effects (CCE-MG) estimator, which has been developed specifically to deal with such "macro panels".[33] Relative to conventional panel techniques it addresses both parameter heterogeneity and cross-section dependence (Pesaran, 2006; Pesaran & Smith, 1995).

However, this comes at a cost: the MG estimator does not model dynamics, and does not account for reverse causality. As such, if there is reverse causality then MG estimates will include the effects of both directions of causality. Indeed, we find that our democracy variable does Granger-cause our revenue variables, suggesting that there is reverse causality. Our estimates for the tax variables are thus best understood as upper bounds.[34] GMM-based estimates, while suffering from problematic instruments in the

[32] In theory a more econometrically ideal approach would be to identify a source of exogenous variation in tax and non-tax revenue by adopting a quasi-experimental design, or by employing instrumental variables. However, over more than a decade of econometric research neither strategy has been implemented fully convincingly, with only highly imperfect instruments available for capturing variation in the shares of tax and non-tax revenue. Studies employing instrumental variables, or exogenous variation, have generally relied on variation in oil prices, but this is a more useful instrument for *resource income* than *resource revenues*, generally implies a focus on *total* resource income, rather than focusing on the more analytically appropriate *share* of resource revenue in total government revenue, and implies a potentially overly narrow understanding of the mechanisms of the underlying relationship of interest. We therefore follow the existing literature on government revenues in concluding that panel data methods offer the best available strategy despite sacrificing an ability to strictly rule out all concerns about endogeneity.

[33] Haber and Menaldo (2011) argue for employing Error Correction Mechanism Regressions. This choice has been challenged on conceptual grounds by Andersen and Ross (2014). More importantly, ECM regressions are more appropriate to their much longer panel, and prove very sensitive when employed with our shorter panel. We thus conclude that they should be excluded. That said, the results are not inconsistent with those reported here. Further information available on request.

[34] That said, more detailed research on the impact of democracy on tax collection has generally suggested either no or very small effects (Cheibub, 1998).

presence of heterogeneous coefficients, do not suffer from this problem, thus making the two methods complementary in triangulating the relationship of interest.

4.3. Logistic regressions focusing on regime transitions

The GMM and CCE-MG estimators offer complementary approaches to estimating the relationship between government revenue and continuous measures of democracy. However, several recent studies have argued for a more narrow focus on the likelihood of *transitions* from autocracy to democracy or, alternatively, from democracy to autocracy.

At the level of theory, Smith (2004), Morrison (2009, 2015) and others have proposed that non-tax revenue may not be inherently pro or anti-democratic, but may simply make any existing form of government more resistant to change. In this case, we would not expect a consistently positive, or negative, relationship between non-tax revenue and democracy, but, instead, an impact of non-tax revenue on the likelihood of political transitions. A focus on regime transitions equally addresses a pragmatic estimation problem: Many resource rich countries were already undemocratic prior to acquiring large-scale resource revenues. In these cases non-tax revenue cannot lead to reduced democracy, but is instead expected to make autocracy more durable and thus reduce the likelihood of democratic transitions (Andersen & Ross, 2014).

We follow Wiens et al. (2014) (henceforth WPC) in estimating a dynamic logit model that captures the impact of the composition of government revenue on the likelihood that a country will transition between democracy and autocracy. Following WPC our prediction is that higher non-tax revenue, and reduced tax reliance, will reduce the likelihood of transitions to democracy. In turn we expect greater tax collection to be associated with an increased likelihood of transitions to democracy, and greater stability in existing democracies.

In order to test these predictions the model differs somewhat from the discussion so far. When we test the impact of *tax_rel* it is:

$$\Pr(\text{Regime}_{i,t}) = \Lambda\Big[\delta\text{Regime}_{i,t-1} + \beta_1\text{Tax}_{\text{rel}\,i,t-1} + \beta_2\text{Regime} \times\text{Tax}_{\text{rel}\,i,t-1} + \beta_3 X_{i,t-1} + \beta_4\text{Regime}_{i,t-1}\times X_{i,t-1} + \lambda_t + e_{i,t}\Big] \quad (3)$$

While separating *tax_rel* into its component parts, *tottax* and *totnontax*, gives us:

$$\Pr(\text{Regime}_{i,t}) = \Lambda\Big[\delta\text{Regime}_{i,t-1} + \beta_1\text{TotTax}_{i,t-1} + \beta_2\text{Regime} \times\text{TotTax}_{i,t-1} + \beta_3\text{TotNonTax}_{i,t-1} + \beta_4\text{Regime} \times\text{TotNonTax}_{\text{rel}\,i,t-1} + \beta_5 X_{i,t-1} + \beta_6\text{Regime}_{i,t-1} \times X_{i,t-1} + \lambda_t + e_{i,t}\Big] \quad (4)$$

In both equations the new measure of democracy is the binary variable *Regime*, described earlier.

The key innovation in WPC, and replicated here, is the inclusion of interaction terms between lagged *Regime* and each of the variables on the right-hand side of the equation. The interaction terms condition the effect of each of the independent variables on the presence, or absence, of democracy. This allows us to identify whether the relationship between the revenue variables and democracy differs between existing democracies and autocracies. The total coefficient on the revenue variables in democracies is thus the sum of the coefficients on the revenue terms, and on the interaction terms between the revenue terms and *Regime*.

As with earlier models, we wish to account for the effects of unobserved country heterogeneity. However, the inclusion of fixed-effects results in dropping from the analysis any country that did not experience a political transition between 1990 and 2010. This leaves only 29 countries, while excluding almost all major resource producers.[35] WPC address this problem by employing a random-effects specification in order to account for unobserved country heterogeneity as far as possible while retaining necessary country coverage. A Hausman test of the validity of adopting the random-effects specification returns a negative value, which is generally interpreted as a valid basis for employing random effects, but still somewhat inconclusive. We thus report results employing both the random-effects and fixed-effects estimations, as we find the key messages are broadly consistent.[36]

5. Results

We first present results employing our array of panel data techniques, before turning to the results of the dynamic logit regressions. Table 3a presents estimates for Pooled OLS, Fixed Effects, Diff- and Sys-GMM and CCE-MG models, with tax reliance (*tax_rel*) as the independent variable. Table 3b presents estimates when we instead include total tax revenue (*tottax*) and total non-tax revenue (*totnontax*) as distinct regressors. The Sys-GMM estimates employ two-step Windmeijer corrected standard errors, and include tests for second-order autocorrelation, as well as the Hansen test of instrument validity, and in all cases we fail to reject the null hypothesis that the results are valid. The CCE-MG tests include the Maddala and Wu panel unit root test, which in all cases rejects non-stationarity at the 1% level.

As can be seen in Table 3a, pooled OLS produces a strongly significant positive coefficient on (lagged) tax reliance, but the coefficient is insignificant when we take country-specific fixed effects into account with the FE model. Turning to the GMM estimates, the results remain insignificant when employing the Diff-GMM estimator, but are again positive and significant at the 5% level when we use Sys-GMM. As discussed above, following Aslaksen (2010), we view the Sys-GMM results to be most valid, and attribute the stronger results to the fact that the Sys-GMM estimator makes use of a wider variety of information and is thus more efficient when the key variables change slowly. Finally, the CCE-MG estimates a similarly positive coefficients on tax reliance, significant at the 5% level.[37]

Table 3b disaggregates tax reliance into tax revenue (*tottax*) and non-tax revenue (*totnontax*). The results again offer support for the political resource curse, and suggest that the positive and significant coefficients on tax reliance are driven primarily by the anti-democratic effect of non-tax revenue. The coefficient on *totnontax* has the expected negative sign in all of the tests, and is statistically significant in the Pooled OLS, Sys-GMM and CCE-MG specifications. The results for *tottax* are more complex. We find the expected positive sign in all but the fixed-effects estimates, but significant only using CCE-MG. By contrast, we find an unexpectedly negative and significant sign for the FE model. This suggests a contrast between models that include information on the long-term relationship in levels, as opposed to a focus exclusively on shorter-term changes. We return to this issue in the discussion to follow.

[35] This includes 16 transitions to autocracy, and 46 transitions to democracy. When the data is extended to 1980–2012 in our robustness checks it includes 24 transitions to autocracy and 63 transitions to democracy, across 41 countries.

[36] While not reported here owing to constraints of space we also test a simple linear probability model without fixed effects, in order to include countries with no variation in the dependent variable: a constant dependent variable can be as informative as a varying one. The results are similar to those for the random effects model, reported below, and thus reinforce the overall story. Detailed results available on request from the authors.

[37] When a trend is included the results are significant and comparable, but the trend itself is insignificant – and we thus do not report it here.

Table 3a
Effect of tax reliance on polity, across different estimation methods, preferred covariates.

	(1) Pooled OLS b/se	(2) FE b/se	(3) Diff-GMM two-step b/se	(4) Sys-GMM two-step b/se	(5) CCE-MG w/o trend b/se
L.Polity_s	0.9491***	0.7457***	0.8103***	0.8299***	
	(0.0097)	(0.0266)	(0.0377)	(0.0355)	
L.Tax_Rel	3.5122***	−0.5047	1.8954	10.7145**	
	(1.0339)	(2.5164)	(6.9337)	(4.9556)	
Tax_Rel					24.5997**
					(9.9183)
L.lgdp	0.1770	0.0178	0.3564	−0.7634	
	(0.1332)	(0.8858)	(1.5514)	(1.1882)	
lgdp					11.2563
					(8.0119)
L.CivilWar	−0.3073	−1.4645	−2.3198	−2.0121	
	(0.4807)	(1.1733)	(1.5542)	(1.4600)	
CivilWar					−0.7931*
					(0.4553)
L.Growthpc	−0.0310	0.0097	0.0036	−0.0104	
	(0.0222)	(0.0263)	(0.0330)	(0.0341)	
Growthpc					−0.1329
					(0.1244)
trend					
Implied Long-Term Effect			9.99	62.99	
AR(1)			0.000	0.000	
AR(2)			0.210	0.225	
Hansen			0.092	0.127	
No. of Obs.	2954	2954	2799	2954	3022
No. of Countries		155	155	155	151
r2	0.945	0.645			

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Implied long-term effect of *Tax_Rel* calculated as $\beta/(1-\omega)$, where ω is the coefficient on the lagged dependent variable, and β is the coefficient on the revenue variable.

Table 3b
Effect of total tax and non-tax revenue on polity, across different estimation methods, preferred covariates.

	(1) Pooled OLS b/se	(2) FE b/se	(3) Diff-GMM two-step b/se	(4) Sys-GMM two-step b/se	(5) CCE-MG w/o trend b/se
L.Polity_s	0.9515***	0.7464***	0.7987***	0.8402***	
	(0.0095)	(0.0265)	(0.0358)	(0.0329)	
L.TotTax	3.2268	−10.0648*	−7.8252	−0.1068	
	(2.3268)	(5.6807)	(15.7795)	(10.6174)	
L.totnontax	−5.9031***	−3.7349	−4.5045	−16.4406**	
	(2.2055)	(4.6339)	(8.4422)	(6.9217)	
TotTax					57.3891**
					(22.7934)
totnotax					−100.8494**
					(47.1368)
L.lgdp	0.0550	0.1496	0.7430	−0.7822	
	(0.1395)	(0.8856)	(1.5271)	(0.9539)	
lgdp					5.4275
					(6.7363)
L.CivilWar	−0.3316	−1.4849	−2.042	−2.0402	
	(0.4809)	(1.1784)	(1.5196)	(1.5044)	
CivilWar					−1.1406**
					(0.5479)
L.Growthpc	−0.0316	0.0126	0.0042	−0.0209	
	(0.0221)	(0.0266)	(0.0329)	(0.0337)	
Growthpc					−0.1798*
					(0.0980)
trend					
Implied Long-Term Effect			22.38	102.88	
AR(1)			0.000	0.000	
AR(2)			0.214	0.230	
Hansen			0.210	0.250	
No. of Obs.	2954	2954	2799	2954	3017
No. of Countries		155	155	155	150
r2	0.945	0.645			

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Implied long-term effect of *Tax_Rel* calculated as $\beta/(1-\omega)$, where ω is the coefficient on the lagged dependent variable, and β is the coefficient on the revenue variable.

Critically, the magnitude of the effects is very substantial. Looking first at the Sys-GMM estimates, the coefficient of 10.45 on *tax_rel* implies that a 10 percentage point increase in *tax_rel* is associated with a short-term increase in *Democracy* of 1.05 percentage points. To place these magnitudes in perspective, a move from the level of tax reliance of Nigeria (average tax_rel = 0.2) to that of Senegal (average tax_rel = 0.9) is associated with a roughly 7 percentage point short-term increase in the level of democracy. We can, in turn, calculate the implied long-term relationship, reported near the bottom of Table 3a, and it is substantially larger: Over the long-term the same increase in tax reliance is associated with an increase in the level of democracy by 45 percentage points – roughly the equivalent of going from Burkina Faso or Algeria to Canada.

Encouragingly, the relationship between non-tax revenue and democracy is of a similar magnitude (Table 3b): A change from the level of non-tax revenue in Nigeria (average non-tax revenue = 22% of GDP) to that of Senegal (average non-tax revenue = 1.5% of GDP) is associated with a short-term increase in democracy of 3 percentage points, and a long-term rise of about 20 percentage points. An increase in non-tax revenue to a level of 43% of GDP – the level in Angola in 2008 – is associated with a long-term decline in democracy of greater than 40 percentage points.

When we turn to the CCE-MG results we see a similar story. The magnitude of the coefficient on *tax_rel* is somewhat smaller, with the shift from Nigeria to Senegal associated with a still substantial 15 percentage point increase in democracy. Meanwhile the coefficient on *totnontax* implies that the same shift from Nigeria to Senegal is associated with an almost 20 percentage point increase in democracy – almost identical to the Sys-GMM estimate.

Table 4a presents results employing the dynamic random and fixed effects logit models. The results estimate the effect of the composition of government revenue on the likelihood of transitions between autocracy and democracy. Columns 1 and 2 report results for random-effects without controls, Columns 3 and 4 random-effects with controls, and Columns 5–8 the same results employing fixed-effects.

The overall results are again generally consistent with expectations. In both the RE and FE models, the coefficient on *tax_rel* is always positive, the coefficient on *totnontax* is always negative, and at least one of the two is statistically significant at conventional levels for every specification. We show below that these results become still stronger when we extend the time series, and thus capture a larger number of transitions. The distinct role of taxation is again more ambiguous, as the sign is reversed between the FE and RE results. However, we will show below that the results are

Table 4a
Effect of tax reliance, total tax and non-tax revenue on regime using dynamic logit models.

	(1) Logit – RE b/se	(2) Logit – RE b/se	(3) Logit – RE b/se	(4) Logit – RE b/se	(5) Logit – FE b/se	(6) Logit – FE b/se	(7) Logit – FE b/se	(8) Logit – FE b/se
L.regime	9.1918***	4.5189***	−2.2683	2.4262	4.4391***	4.4461***	6.4427	7.4486**
	(1.5327)	(0.8771)	(3.6946)	(3.0741)	(1.5608)	(1.0677)	(4.9639)	(3.6915)
L.Tax_Rel	2.0800**		1.1210		6.3509**		7.5869**	
	(0.9073)		(1.0663)		(2.8981)		(3.2035)	
L.regimextaxrel	−0.4291		2.2872		1.7350			
	(1.9296)		(1.9465)		(1.9495)			
L.TotTax		−5.8995*		−6.9451*		13.1635		19.1017
		(3.2053)		(3.6907)		(14.0760)		(15.6223)
L.TotNonTax		−10.7868***		−11.695**		−12.6319		−15.6038
		(3.9705)		(5.3393)		(8.8320)		(10.1852)
L.regimextottax		26.8080***		20.6205**		−0.1911		−2.2981
		(6.6025)		(8.1548)		(8.3616)		(9.6348)
L.regimextotnontax		14.3523*		8.5020		−0.4632		−0.5772
		(7.5359)		(7.6031)		(7.4342)		(9.5025)
L.lgdp			−0.3088	0.0859			0.2762	0.0227
			(0.2227)	(0.2601)			(1.9784)	(1.9905)
L.Civil_War			−0.1221	−0.2754			−0.7225	−0.8092
			(0.4570)	(0.4577)			(0.6222)	(0.6147)
L.Growthpc			−0.0228	−0.0250			−0.0033	−0.0180
			(0.0304)	(0.0302)			(0.0393)	(0.0406)
L.regimexlgdp			1.2500***	0.4164			−0.2174	−0.3162
			(0.3882)	(0.4443)			(0.5088)	(0.5271)
L.regimexcivilwar			−1.7908**	−1.5057**			−1.1524	−1.1559
			(0.7641)	(0.7629)			(0.8562)	(0.8553)
L.regimexgrowthpc			0.1080	0.1146			0.1440	0.1557*
			(0.0717)	(0.0722)			(0.0890)	(0.0887)
Aggregate Effects in Democracies								
β1 + β2	1.5600		2.7772*		6.3349**		7.6326**	
	(1.7208)		(1.6430)		(3.1731)		(3.7359)	
β1 + β3		20.9086***		13.6753*		12.9724		16.8036
		(5.77646)		(7.2233)		(15.1837)		(16.6575)
β2 + β4		3.5654		−3.1929		−13.0952		−16.1811
		(6.4536)		(5.4354)		(9.7140)		(11.0166)
Random Effects Model Descriptors								
σ_μ	0.0001925	0.000551	0.00161	0.001647				
ρ	1.13e−08	9.22e−08	7.89e−07	8.24e−07				
N	2771	2771	2660	2660	480	480	468	468
N_g	181	181	181	181	29	29	29	29

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. "*β1 + β2*" captures the joint significance of *tax_rel* and *regime x taxrel*, and thus captures the impact of *tax_rel* on *regime* in democracies. "*β1 + β3*" captures the joint significance of *tottax* and *regime x tottax*, while *β2 + β4*" captures the joint significance of *totnontax* and *regime x totnontax*.

Table 4b
Likelihood of transitions from autocracy to democracy.

	Level of non-tax revenue as % of GDP				
	2%	5%	10%	20%	30%
Pr(Democracy) at *t*	0.0345	0.0241	0.0132	0.0039	0.0011
95% confidence interval (Upper)	0.0515	0.0348	0.0226	0.0102	0.0041
95% confidence interval (Lower)	0.0175	0.0134	0.0037	−0.0025	−0.0018
	Level of Tax Reliance (Tax Revenue share of Total Gov't Revenue)				
	0.2	**0.4**	**0.6**	**0.75**	**0.9**
Pr(Democracy) at *t*	0.0118	0.0153	0.0198	0.0240	0.0290
95% confidence interval (Upper)	0.0256	0.0277	0.0301	0.0343	0.0441
95% confidence interval (Lower)	−0.0018	0.0030	0.0095	0.0137	0.0140

Notes: Calculated based on Columns 3 and 4 of Table 4a, using *margins* command in stata, with regime = 0, civil_war = 0 and other variables set to their mean values excluding OECD countries.

more in line with expectations once we account for a longer lag between changes in revenue and their political impacts.

Adding additional nuance are the interaction terms. While the results described so far capture the relationship between changes in the composition of government revenue and the likelihood of transitions from autocracy to democracy, the interaction terms capture the distinct relationship between the same variables in existing democracies. We again find a positive impact of *tax_rel* on democracy (row $\beta 1 + \beta 2$), indicating that greater tax reliance has a stabilizing effect in existing democracies. However, the driver of this relationship may be different in democracies than in autocracies. Whereas non-tax revenue is strongly associated with lower levels of democracy in autocracies, in democracies the coefficient on *totnontax* is negative and significant in the fixed-effects estimates, but highly insignificant in the random-effects estimates (though it continues to have a negative sign) (row $\beta 2 + \beta 4$). By contrast, tax revenue, which is insignificant in autocracies, is positive in all cases in democracies, and significant in both of the RE models (row $\beta 1 + \beta 3$).

Because the logit estimates are non-linear, we separately calculate the magnitude of the implied effects, reported in Tables 4b and 4c, drawing on the preferred results in columns 3 and 4 of Table 4a. Table 4b reports the likelihood that a country that is an autocracy in period *t-1* will become a democracy at time *t*, at different levels of non-tax revenue and tax reliance. Again the magnitude of the effects is substantial. With all other variables at their means for developing countries, an autocratic country with non-tax revenue of 2% of GDP has a 3.5% probability of transitioning to democracy in a given year. By contrast, an autocratic country with non-tax revenue of 10% of GDP (e.g. Malaysia) has only a 1.3% probability of a democratic transition, while non-tax revenue of 20% of GDP (e.g. Gabon or Chad) reduces that probability to 0.4%. Put differently, the likelihood of a democratic transition declines by almost a third as non-tax revenue increases to 10% of GDP, and by more than 80% when non-tax revenue increases to 20% of GDP.

Table 4c reverses the question of interest, and reports the likelihood that a democratic country will transition to autocracy at time *t*, for different levels of non-tax revenue and tax reliance. Here the pattern is somewhat different, with changes in tax reliance having a larger impact than changes in non-tax revenue alone – reflecting the enhanced importance of taxation, and the role of stronger institutions in moderating the anti-democratic effects of natural resource wealth. The results suggest that a democratic country with tax reliance of 0.9 (e.g. the Philippines) and other variables at their developing country means, has only about a 0.3% likelihood of transitioning to autocracy in any given year, while an otherwise identical country with tax reliance of 0.2 (e.g. Nigeria) has a 2.1% likelihood of becoming an autocracy – seven times more likely. By contrast, going from non-tax revenue of 2% of GDP to non-tax revenue of 20% of GDP only roughly doubles the likelihood of a transition to autocracy. This suggests that it is weak tax systems that may be an important predictor of the weakness of existing democracies.

While the results so far present relatively consistent support for the existence of a political resource curse, Ross (2004) and Andersen and Ross (2014) have argued for employing longer lags on the explanatory variables in dynamic models, in order to more accurately model the time period over which changes in revenue may impact levels of democracy. Tables 5a and 5b correspondingly report results using one, three and five year lags on the independent variables for the Sys-GMM and the logit estimations, respectively.

The results are striking, as the key coefficients on *tax_rel* and *totnontax* become almost universally larger with a longer lag, precisely as theory predicts. In addition, we find that while the coefficients on *tottax* become universally more positive with longer lags. For the fixed effects logit this causes the coefficient on *tottax* to become strongly positive and significant with a longer lag, while in the random effects logit the surprisingly negative coefficient on *tottax* from the earlier results disappears. These results are consistent with longer lags better capturing the causal process of interest.

Having presented detailed evidence of the existence of the political resource curse, we conclude the core analysis by further exploring the mechanisms underlying this relationship. The paper began with the contention that the mixed results of earlier studies can in part be explained by researchers' frequent reliance on measures of *resource income* as the key explanatory variable, rather than more theoretically appropriate measures of the composition of *government revenue*. To demonstrate this point more systematically, Tables 6a and 6b directly compare the results of each of our models when employing alternative independent variables from the literature: (a) *tax_rel*, (b) *tottax* and *totnontax*, (c) total oil income per capita (*OilIncome*), (d) total oil income as a share of GDP (*OilIncShare*), and (e) total resource income as a share of GDP (*ResIncShare*).[38]

The results are consistent in their broad message. For each of our core models we find statistically significant evidence of the political resource curse when relying on measures of the composition of government revenue, but find no statistically significant evidence of the political resource curse when relying on the various measures of resource income that have been employed elsewhere in the literature.[39] The latter always have the expected

[38] *OilIncome* is from Haber and Menaldo (2011), while *OilIncShare* and *ResIncShare* are constructed using the matching GDP series in WPC (2014).

[39] The large (though highly insignificant) coefficients on *TotOilIncome*, *OilIncShare* and *ResIncShare* in the CCE-MG regressions reflect the fact that the majority of countries have extremely low values for these variables relative to the dependent variable. Since CCE-MG averages country-specific coefficients, the large coefficient is not surprising given the overall insignificance of the results. If we limit the sample to countries with oil income share greater than 1% of GDP then the coefficient returns to a magnitude comparable to all other results (-20.5), but remains highly insignificant (p=0.875).

Table 4c
Likelihood of transitions from democracy to autocracy.

	Level of non-tax revenue as % of GDP				
	2%	5%	10%	20%	30%
Pr(Democracy) at *t*	0.0039	0.0043	0.0051	0.0071	0.0099
95% Confidence Interval (Upper)	−0.0001	0.0002	−0.0002	−0.0055	−0.0171
95% Confidence Interval (Lower)	0.0079	0.0084	0.0104	0.0197	0.0370
	Level of Tax Reliance (Tax Revenue share of Total Gov't Revenue)				
	0.2	**0.4**	**0.6**	**0.75**	**0.9**
Pr(Democracy) at *t*	0.0213	0.0124	0.0072	0.0048	0.0032
95% Confidence Interval (Upper)	−0.0170	−0.0036	0.0004	0.0004	−0.0004
95% Confidence Interval (Lower)	0.0594	0.0283	0.0039	0.0091	0.0067

Notes: Calculated based on Columns 3 and 4 of Table 4a, using *margins* command in stata, with regime = 0, civil_war = 0 and other variables set to their mean values excluding OECD countries.

Table 5a
Effect of tax reliance, total tax and non-tax revenue on polity, using different lags.

	(1) Sys-GMM two-step b/se	(2) Sys-GMM two-step b/se	(3) Sys-GMM two-step b/se	(4) Sys-GMM two-step b/se	(5) Sys-GMM two-step b/se	(6) Sys-GMM two-step b/se
Lag Length	λ = 1	λ = 3	λ = 5	λ = 1	λ = 3	λ = 5
Lλ.Polity_s	0.8299***	0.5597***	0.4395***	0.8402***	0.5605***	0.4515***
	(0.0355)	(0.0821)	(0.1052)	(0.0329)	(0.0754)	(0.0960)
Lλ.Tax_Rel	10.7145**	16.0337**	15.3247*			
	(4.9556)	(6.5186)	(8.2139)			
Lλ.TotTax				−0.1068	37.2723	32.4525
				(10.6174)	(28.9490)	(34.6941)
Lλ.totnotax				−16.4406**	−23.2742	−34.2044**
				(6.9217)	(14.4920)	(16.6445)
AR(1)	0.000	0.213	0.871	0.000	0.260	0.739
AR(2)	0.225	0.186	0.288	0.230	0.192	0.260
Hansen	0.127	0.282	0.277	0.250	0.368	0.160
N	2954	2681	2404	2954	2681	2404
N_g	155	153	152	155	153	152

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Windmeijer standard errors in parentheses. Columns (1), (4) use lags t-1; columns (2), (5) use lags t-3; columns (3), (6) use lags t-5. Results include a lagged dependent variable and standard controls for lgdp, civil_war and growthpc.

sign, but fall short of standard thresholds of statistical significance This offers strong support for the idea that the political resource curse is, indeed, driven primarily by changes in the composition of government revenue.[40] It also raises a question: given a significant correlation between resource income and non-tax revenue, what explains the divergent results? That is, why does focusing on non-tax revenue more precisely capture the relationship of interest than focusing on resource income? This is a broader question than can be fully answered here, and warrants further research. However, an initial exploration of the data suggests that many of the largest oil producers are comparatively successful in translating resource income into resource revenues – and are also most autocratic.[41] By contrast, a number of large mineral producers are relatively ineffective at translating resource income in resource revenues – and are also comparatively democratic.[42] The latter also seems to apply, to a lesser extent, to some mid-sized oil producers.[43] The greater ability of countries to translate oil wealth into government revenues, in turn, offers an explanation for growing evidence that the political resource curse has, in fact, been primarily a political "oil curse", with mineral wealth having a more muted impact, if any (Ross, 2012, 2015).

Finally, while Tables 6a and 6b report results using a one-year lag on the independent variables, additional results using 3 and 5 years lags are presented in the Appendix, Tables A1a–d. The overall pattern of results is the same, with the exception that the coefficients on the resource income variables become significant and more comparable to the coefficients on the revenue variables in the logit models with a five-year lag. One potential explanation, consistent with the story told so far, is that the resource income variables come to more closely resemble the resource revenue variables with a longer lag. In new areas of resource production firms often pay relatively limited revenues to government initially – owing to short term tax relief, or extensive deductions for initial capital investments – while over a longer time period government revenues may begin to catch up to resource production.[44]

6. Robustness checks

The core results indicate that increased non-tax revenue, and decreased tax reliance, are strongly associated with the likelihood that a country will transition to, or remain, a democracy. However, cross-country econometric results are notoriously sensitive, and we correspondingly test the robustness of the results to changes in control variables, the calibration of the Sys-GMM model, the use of alternative dependent variables, reliance on central government data only, the use of a longer (but less complete) time series and the exclusion of different groups of countries. In all cases the

[40] Earlier studies have sometimes reported positive results linking these measures to democracy. Our interpretation, after careful analysis, is that earlier evidence is comparatively sensitive to specific choices about variables, methods and sample, whereas the results here when focusing on the composition of government revenue are far more robust to a wide variety of specifications. Additional information is available on request.

[41] Examples include Angola, Bahrain, Kuwait, Libya, Oman, Saudi Arabia and the United Arab Emirates

[42] Examples include Ghana, Guyana, Mongolia, Peru and Zambia.

[43] Examples include Chad, Gabon, Azerbaijan and Kazakhstan.

[44] And, indeed, the simple correlation between Resource Income and Non-Tax Revenue is substantially higher when using a lagged version of Resource Income (0.62 vs. 0.50)

Table 5b
Tax reliance, total tax and non-tax revenue on regime using random and fixed effects logit models and different lags.

	(1) Logit – RE b/se λ = 1	(2) Logit – RE b/se λ = 3	(3) Logit – RE b/se λ = 5	(4) Logit – RE b/se λ = 1	(5) Logit – RE b/se λ = 3	(6) Logit – RE b/se λ = 5	(7) Logit – FE b/se λ = 1	(8) Logit – FE b/se λ = 3	(9) Logit – FE b/se λ = 5	(10) Logit – FE b/se λ = 1	(11) Logit – FE b/se λ = 3	(12) Logit – FE b/se λ = 5
Lλ.Tax_Rel	1.1210	2.7481*	5.7188***				7.5869**	4.5646*	8.1574***			
	(1.0663)	(1.5038)	(1.7969)				(3.2035)	(2.5892)	(2.9519)			
Lλ.regimextaxrel	1.6562	2.8830	3.8521				0.0460	0.1386	−0.6973			
	(1.9552)	(2.1759)	(2.4675)				(2.4217)	(2.0552)	(2.4238)			
Lλ.TotTax				−6.9451*	−6.3527	1.8472				23.3852	31.0962**	55.4783***
				(3.6907)	(4.8319)	(5.9654)				(15.6515)	(13.0215)	(15.8134)
Lλ.TotNonTax				−11.6949**	−14.7129***	−17.2846***				−14.9969	−11.4022	−16.4315
				(5.3393)	(5.5018)	(6.2973)				(10.1621)	(8.1780)	(10.4225)
Lλ.regimextottax				20.6205**	22.0937***	18.6939**				−3.0542	−5.5134	−16.7679
				(8.1548)	(8.3163)	(9.2724)				(9.6120)	(8.3539)	(10.5746)
Lλ.regimextotnontax				8.5020	9.4119	6.8268				−0.8904	−1.7791	−6.0835
				(7.6031)	(7.1837)	(8.8888)				(9.5636)	(7.6889)	(8.2861)
Aggregate effects in democracies												
β1 + β2	2.777217*	5.631105***	9.5709***				7.632901**	4.703199	7.460131**			
	(1.643015)	(2.116353)	(2.4695)				(3.735864)	(3.06411)	(3.537407)			
β1 + β3				13.67533*	15.74099**	20.54118**				20.33095	25.58278*	38.7104**
				(7.223274)	(7.548232)	(8.688208)				(16.72951)	(13.5413)	(15.96467)
β2 + β4				−3.192879	−5.300943	−10.45787				−15.88728	−13.18135	−22.51505*
				5.435374)	(5.115675)	(6.996367)				(11.05537)	(9.054927)	(12.76262)
Random Effects Model Descriptors												
σ_μ	0.0016111	2.329794	3.777649	0.00164	2.0214	3.375611						
ρ	7.89e−07	0.6226266	0.8127	8.24e−07	0.55397	0.775965						
N	2660	2321	1984	2660	2321	1984	468	367	298	468	367	298
N_g	181	178	176	181	178	176	29	26	24	29	26	24

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Columns (1), (4), (7), (10) use lags t-1; columns (2), (5), (8), (11) use lags t-3; columns (3), (6), (9), (12) use lags t-5. Results include a lagged dependent variable and standard controls for lgdp, civil_war and growthpc. "*β1 + β2*" captures the joint significance of *tax_rel* and *regime x taxrel*, "*β1 + β3*" captures the joint significance of *tottax* and *regime x tottax*, and "*β2 + β4*" captures the joint significance of *totnontax* and *regime x totnontax*.

Table 6a
Comparing results using resource revenue or resource income as independent variables – panel methods.

	(1) Sys-GMM two-step b/se λ = 1	(2) Sys-GMM two-step b/se λ = 1	(3) Sys-GMM two-step b/se λ = 1	(4) Sys-GMM two-step b/se λ = 1	(5) Sys-GMM two-step b/se λ = 1	(6) CCE-MG group w/ trend b/se λ = 0	(7) CCE-MG group w/ trend b/se λ = 0	(8) CCE-MG group w/ trend b/se λ = 0	(9) CCE-MG group w/ trend b/se λ = 0	(10) CCE-MG group w/ trend b/se λ = 0
L.Polity_s	0.8299***	0.8374***	0.8410***	0.8442***	0.8422***					
	(0.0355)	(0.0331)	(0.0378)	(0.0374)	(0.0366)					
Lλ.Tax_Rel	10.7145**					24.5997**				
	(4.9556)					(9.9183)				
Lλ.TotTax		0.6663					57.3891**			
		(10.8254)					(22.7934)			
Lλ.	TotNonTax		−14.4283**					−100.8494*		
		(6.8737)					(47.1368)			
Lλ.TotOilInc			−0.0002					−1.1706		
			(0.0002)					(1.2024)		
Lλ.	OilIncShare				−1.6184					−9325.9767
				(1.2694)					(6093.5250)	
Lλ.	ResIncShare					−0.9820				
5519.4511										
					(1.3998)					(5720.8841)
AR(1)	0.000	0.000	0.000	0.000	0.000					
AR(2)	0.225	0.229	0.855	0.841	0.841					
Hansen	0.127	0.834	0.149	0.153	0.151					
N	2954	2954	2319	2319	2319	3022	3017	2316	2316	2316
N_g	155	155	154	154	154	150	150	150	150	150

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Windmeijer standard errors in parentheses for GMM results. Results include standard controls *lgdp*, *growthpc* and *civil_war*.

Table 6b
Comparing Results Using Resource Revenue or Resource Income as Independent Variables – Logit.

	(1) Logit – RE b/se Revenue = Tax_Rel	(2) Logit – RE b/se Revenue = TotNonTax	(3) Logit – RE b/se ResourceIncome = OilIncome	(4) Logit – RE b/se ResourceIncome = OilShareGDP	(5) Logit – RE b/se ResourceIncome = ResShareGDP	(6) Logit – FE b/se Revenue = Tax_Rel	(7) Logit – FE b/se Revenue = TotNonTax	(8) Logit – FE b/se ResourceIncome = OilIncome	(9) Logit – FE b/se ResourceIncome = OilShareGDP	(10) Logit – FE b/se ResourceIncome = ResShareGDP
L.Revenue	1.1210	−11.6949**				7.5870**	−15.6038			
	(1.0663)	(5.3393)				(3.2035)	(10.1852)			
L.regimexRevenue	1.6562	8.5020				0.0457	−0.5772			
	(1.9552)	(7.6031)				(2.4218)	(9.5025)			
L.ResourceIncome			−0.0006	−1.0338	−2.3274			−0.0026	−7.6127	−7.3238
			(0.0009)	(2.7958)	(3.0249)			(0.0033)	(6.4530)	(6.3449)
L.regimexResourceIncome			0.0004	0.4172	1.8632			−0.0010	−3.9970	−3.2298
			(0.0010)	(2.9967)	(3.3048)			(0.0025)	(5.3707)	(5.4680)
Aggregate Effects in Democracies										
$\beta1 + \beta2$	2.777217*	−3.192879	−0.0002067	−0.6166	−0.4641	7.6326**	−16.18105	−0.0036419	−11.6098	−10.5537
	(1.643015)	(5.435374)	(0.0004395)	(1.0991)	(1.3894)	(3.735883)	(11.01656)	(0.0046418)	(9.1982)	9.0373
Random effects model descriptors										
σ_μ	0.0016111	0.0016465	0.0022415	0.002265	0.002265					
ρ	7.98e−07	8.24e−07	1.53e−06	1.56e−06	1.56e−06					
N	2660	2660	2390	2388	2388	468	468	453	453	453
N_g	181	181	158	158	158	29	29	28	28	28

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01. "Revenue" represents either Tax_Rel (columns 1 and 6) or TotNonTax (columns 2 and 7), while "ResourceIncome" represents *TotalOilInc* (3, 8), *OilIncShare* (4,9), or *ResIncShare* (5, 10). All results include standard controls for *lgdp*, *civil_war* and *growthpc* – while columns 2 and 6 also include the variable TotTax, as in previous tables. "$\beta1 + \beta2$" captures the joint significance of the *Revenue* and *regime x Revenue* or of *ResourceIncome* and *regime x ResourceIncome*.

results prove robust to these alternatives, which are reported in detail in the Appendix and described briefly here.

Tables A2a and A2b report results adding our standard control variables to our core models one by one, and then also adding controls for population and regional democratic diffusion, both of which are employed by Haber and Menaldo (2011). The results prove consistently robust to different groups of control variables.[45] Table A3 then tests the robustness of the results to slightly different specifications of the Sys-GMM model, varying in the number of lags employed, and whether control variables are treated as exogenous or endogenous. Again the results prove robust to these alternatives. Table A4 tests the results when employing data exclusively at the central government level, instead of the preferred ICTD GRD dataset, which employs general government data where available. The results remain generally significant, but modestly weaker, which is consistent with higher overall data quality revealing a stronger and more consistent relationship.

Tables A5a–c test the robustness of our results to employing alternative measures of democracy and accountability. We re-run our Sys-GMM results, with multiple lag lengths, when employing several other common measures: the Democracy measure from the International Country Risk Guide (*icrg_dem*), the Freedom House measure of democracy (*fh_dem*), and the Accountability measure from the World Bank's World Governance Indicators (*wgi_acc*). In turn, we re-run our logit results using an alternative binary measure of democracy (*regime_polity_60*), constructed by coding countries as democracies if they achieve a scaled polity score of 60 out of 100 or above.[46] These measures are all significantly less desirable than the measures used in our core results. Yet we nonetheless find statistically significant relationships, with the expected sign, in more than half of the specifications, and spanning all four alternative measures of democracy and accountability, thus offering further confidence in the overall results.

The core results reported so far employ a dataset that begins in 1990, as this is the period during which data coverage is most complete, high quality and representative. That said, Tables A6a–f explore the robustness of the results to employing a longer time series. For the Sys-GMM and CCE-MG tests we expect the results to grow gradually weaker as the time series is extended, for two reasons: first, the declining quality of the data, and extent of data coverage, and, second, evidence from Andersen and Ross (2014) that the relationship between oil wealth and democracy has been strongest since about 1985, as national governments have exercised increasing control over resource extraction. This is precisely what we find: All of the results remain statistically significant in almost all cases, but grow progressively weaker. Meanwhile, the logit results grow noticeably stronger with the longer time series, most likely because this introduces a larger number of autocracy-democracy transitions into the dataset.

Finally, Tables A7a–i report a battery of results exploring robustness to excluding particular groups of countries. For each of our estimation methods we begin by excluding the 11 members of OPEC, and then an 'expanded OPEC' that includes recently withdrawn Ecuador and Venezuela. We subsequently exclude, in turn, countries from the Middle East and North Africa (MENA region) and Eastern Europe and Central Asia (EECA), both as defined by the World Bank. Both are home to significant resource producers, while the latter has undergone major governance changes since the collapse of the Soviet Union. Finally, we also re-estimate our results when excluding OECD countries, and when excluding countries with populations below either 250,000 or 1,000,000.

[45] We exclude Regional democratic diffusion from the CCE-MG estimations, as the CCE-MG estimator already accounts for cross-sectional correlations.

[46] This is equivalent to a score of 2 or above (on a scale -10 to 10) on the polity2 variable as coded in the Polity IV dataset.

Essentially all of the results are fully robust to these changes in the sample, though the results are modestly weaker when we exclude major groups of oil producers. This is to be expected, but also highlights the importance of the ICTD GRD data, which most dramatically improves data coverage for resource rich states.

7. Discussion

Overall, we find strong evidence for the existence of a political resource curse. Across the vast majority of the tests we find *both* a statistically significant positive relationship between tax reliance and democracy *and* a statistically significant negative relationship between total non-tax revenue and democracy. At least one of the two is significant in every model specification, and virtually every robustness test, across each of the Sys-GMM, CCE-MG, RE logit and FE logit estimations. Consistent with earlier arguments, the strength of the results increases as the length of the lag on the revenue variables increases. The broad results are robust to alternative control variables, employing only central government data, alternative measures of democracy, the extension of the time series and systematically excluding particular groups of countries.

Critically, the magnitude of the effects is large – but still plausible – and comparable across several very different estimators. On balance the results suggest that moving from the resource wealth of Senegal (average non-tax revenue of 1.5% of GDP, tax reliance of 0.9) to that of Nigeria (average non-tax revenue of 22% of GDP, tax reliance of 0.22) is associated with a reduction in the level of democracy of roughly 20 to 45 percentage points (dynamic panel estimates), or, alternatively, is associated with a reduction in the likelihood of a transition from autocracy to democracy of about 70% (logit estimates). In turn, increasing resource revenue to the level of Angola (above 40% of GDP) is associated with a reduction in the level of democracy of 40–60 percentage points, and a reduction the likelihood of a transition to democracy of about 85%. These estimates suggest that resource wealth may be the strongest single cross-country predictor of democratization identified by existing research. They correspondingly imply the need for a focus on potential governance challenges in the growing number of low-income countries that are, or will soon be, producing oil for international markets.

In offering strong support for the existence of a political resource curse, the results reported here call recent findings to the contrary into question. Following the work of Andersen and Ross (2014) and WPC (2014), our results suggest that the negative findings reported by Haber and Menaldo (2011) are reflective of their particular – and, in our view, problematic – choices about time period, independent variables and estimators. By focusing on a more contemporary time period, the composition of government *revenue* and estimators that more clearly match the expected relationship, we provide what we believe to be the most systematic evidence to date of the political resource curse.

In turn, our logit results speak directly to the recent work of Morrison (2009, 2015) who, like us, tests the relationship between tax revenue, non-tax revenue and democracy. In his tests Morrison finds that non-tax revenue does not have an inherently anti-democratic effect, but stabilizes both autocracies and democracies alike. In turn, he argues that taxation is not democratizing, but destabilizing, increasing the likelihood of regime transitions. The arguments are provocative, and supported by case studies, but are also open to question: they run counter to the majority of existing research and, as noted in Table 1, the quantitative results are based on highly problematic earlier data.

Consistent with these concerns, our results appear to contradict Morrison's arguments. With respect to non-tax revenue, Morrison's argument predicts an anti-democratic effect in existing autocracies, and a pro-democratic effect in existing democracies. By contrast, we find consistent evidence of an anti-democratic effect of non-tax revenue in autocracies, and an insignificant or anti-democratic effect of non-tax revenue in democracies. This mirrors earlier arguments that the anti-democratic effects of non-tax revenue are likely to be more muted in democracies, owing to the countervailing effects of stronger institutions, checks and balances (Mehlum, Moene, & Torvik, 2006; Andersen & Aslaksen, 2013; Wiens et al., 2014).

With respect to tax revenue, Morrison predicts a pro-democratic effect of taxation in autocracies, and an anti-democratic effect in existing democracies. By contrast, we find weak support for a pro-democratic effect in autocracies, and no support at all for an anti-democratic effect in democracies. In fact, we find the opposite: A substantively large positive effect of taxation on democracy in existing democracies in our random-effects logit models.[47] Again, this is consistent with more conventional arguments in the existing case-study literature, which suggest that democratic institutions may amplify the pro-democratic potential of taxation by providing greater potential for popular collective action and "tax bargaining" (Levi, 1988; Prichard, 2015).

While the results thus offer support for the existence of a political resource curse, the parallel goal of this paper has been to shed light on the mechanisms underpinning this relationship. We find that, across multiple alternative estimators, the results are consistently larger and more significant when employing measures of the composition of government revenue, as opposed to more common measures of resource income. This has several implications. First, it helps to explain why oil wealth has been more consistently linked to autocracy than resource wealth from mining: mineral wealth generally produces significantly less government revenue per unit value of production, particularly in low-income countries. Second, it increases our understanding of the likely causes of the resource curse by focusing attention on mechanisms that occur via government revenue – most notably, expanded public spending and patronage, expanded security spending, expanded scope for the enrichment and empowerment of state elites and reduced reliance on the taxation of citizens. Most broadly, it argues for future research that focuses on government revenue, rather than the more common focus on resource income more broadly. There continue to be plausible arguments for focus on resource income: data continues to have more complete coverage, while it has so far proven more amenable to locating sources of exogenous variation over time. However, with improvements in the coverage and quality of revenue data the potential advantages of relying on resource income data are, in our view, outweighed by the benefits of employing revenue data that more precisely matches the theory underpinning the political resource curse.

Our results also speak to two ongoing debates in the literature. First, is the political resource curse largely about long-term effects in levels or about short-term changes (Ross, 2015)? By employing a variety of econometric estimation techniques side by side we gain leverage over this question, and the pattern of results supports an understanding of the political resource curse as a longer-term relationship. All of the results are stronger using longer lags on the independent variables and when employing Sys-GMM, CCE-MG and logit models that incorporate information both on within country variation over time and cross-country differences in levels. By contrast, models that focus exclusively on short-term within country variation – Fixed Effects, Diff-GMM[48] – yield results that

[47] Which is also replicated in a simple linear probability model, noted above.

[48] While we do not report results using the ECM regressions employed by Haber and Menaldo (2011), owing to their sensitivity with a shorter time series, the results are consistent with the story here, with the revenue variables taking the expected sign, but the coefficients generally statistically insignificant.

are frequently insignificant and smaller in magnitude, though our fixed-effects logit results remain significant.

Second, is the resource curse best captured by relating the composition government revenue to *levels of democracy*, or to the *likelihood of transitions* between autocracy and democracy (Wiens et al., 2014)? We find significant support for both versions of the question. That said, we generally favor a focus on transitions. By systematically distinguishing the effects of changes in the composition of revenue between autocracies and democracies these models offer additional nuance. As importantly, they deal most effectively and explicitly with the fact that most major resource producers were autocracies prior to becoming resource producers, and that resource wealth has correspondingly blocked transitions, rather than reducing the level of democracy, in many cases. And, indeed, our results using dynamic logit models remain robust in both random and fixed-effects specifications.

Finally, a note is warranted on the comparatively ambiguous results relating total tax revenue to democracy. The most common reading of theory predicts that tax revenue should be associated with higher levels of democracy, with increases in taxation temporally preceding increases in democracy (e.g. Ross, 2004). However, while some of our results show this relationship, the bulk of our results for *tottax* are insignificant. While our results may thus be read as a repudiation of theory, recent case study research suggests that it may, in fact, be comparatively simplistic readings of theory that are flawed – with the mixed quantitative results here reflecting these limitations.

Prichard (2015) presents evidence that the impact of increased taxation on accountability tends to be comparatively long term, while this relationship may not follow a straightforward sequence; increased taxation may precede accountability, but there may equally be short-term *decreases in taxation* that subsequently generate pressure for improvements in accountability (see also Prichard, 2016b). In turn, he suggests that such comparatively long-term and indirect processes will be more common in more restrictive political environments. Econometrically, this research predicts (i) a positive long-term relationship between taxation and democracy, but (ii) significant difficulty in identifying a relationship econometrically over the short-term using within-country estimators, and (iii) clearer results for more democratic states, where "tax bargaining" is more likely to be linear and immediate. The results presented here are consistent with these predictions: Our long-term estimators (Pooled OLS and CCE-MG) offer strong support for a positive relationship between taxation and accountability, all of the results are more positive (though often still insignificant) with longer lags, and we find generally stronger evidence for a tax-democracy link in existing democracies. While we do not view the results here as strong evidence *in favor* of a tax-democracy link, they do seem to offer support for a more nuanced reading of the causal processes potentially underpinning any such relationship.

8. Conclusions

The premise of this paper is that the expanding econometric literature on the political resource curse has suffered from a reliance on explanatory variables that only imperfectly match the theoretical propositions of interest, due to stark weaknesses in cross-country government revenue data. As with many areas of development research, the most useful strategy for generating improved results thus lies not in more complex methods, or more complex hypotheses, tested using existing data. Instead, we make more progress by improving the quality of underlying data, allowing us to specify more theoretically precise propositions, which we then subject to a variety of complementary econometric tests. By doing so we find what we believe to be the most robust available evidence of the existence of a political resource curse, while shedding new light on the causal mechanisms underlying this relationship.

9. Conflict of Interest

None declared.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.worlddev.2018.05.014.

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